

I have dried about two thousand plants, among which are several new genera, and some species hitherto undescribed.

I sometimes amuse myself with electricity. This country is so dry, that the experiments often succeed without any stand of bitumen, pitch, silk, glass, &c. Our carpets and beavers are mostly sufficient to retain the electrical virtue, and prevent its spreading to the floor. Ten men standing upright, one before the other, have been made electrical, and, being touched, have produced sparks.

XLII. *Some Observations proving, that the Foetus is in part nourished by the Liquor Amnii.* By Malcolm Fleming, M. D.

Read Nov. 13,
1755.

BESIDES the mysterious affair of generation, besides the manner of the formation of the embryo, and the changes it undergoes, while it continues extremely small, concerning all which we shall ever remain very much in the dark; there are not a few things relating even to the mature foetus, which create matter of dispute amongst anatomists and physiologists. Of these the manner of its nourishment *in utero* is not the least important.

The present state of the controversy concerning this matter turns on the following precise point, to wit, whether the foetus in utero be nourished solely by the blood, which is transmitted to it through the umbilical

umbilical cord ; or whether it is likewise nourished in part by the liquor amnii, in which it swims. I have said the present state of the controversy ; for I believe very few, if any at all, will maintain now-a-days with Claudius de la Courvée and Stalpart Vander-wiel, that the whole of its nourishment is conveyed by the mouth, and none at all by blood-vessels.

Both sides of the 'question have been espoused by able writers, and supported with great acuteness, erudition and industry. And as there still subsists a division amongst candid and intelligent inquirers concerning this matter, it would appear, that some decisive fact, some demonstrative argument hath hitherto escaped notice, which, if clearly known and duly attended to, would have by this time ended the dispute, and rendered the proposition, which declares the truth, on which-soever of the sides, an object of science ; which before was only matter of opinion.

An observation, which fell in my way two years ago, together with some subsequent ones, which I purposely made in pursuance of the hint suggested by the first, will, I hope, enable all, who candidly consider the facts themselves, and the consequences that naturally flow from them, to determine certainly, on which side the truth lies.

Before I relate my observations, let me lay down a necessary preliminary, which I observe the writers on both sides are either explicitly or implicitly agreed in ; to wit, that if it be clearly made out, that the liquor amnii is naturally received into the mouth, stomach, and intestines of the fœtus, swimming in it ;

it; in that case we are to conclude, that the foetus is in some part nourished by it. The whole tract of the alimentary passage abounds with absorbent vessels in the foetus, more than in the adult animal; and especially the small intestines have lacteals plentifully opening into them. The liquor amnii is concretable by heat, like the white of an egg; which characteristic in animal juices is, I believe, denied by none to be a proof of their alimentary nature. To such as will not grant this postulatum, if any such there be, this paper is not addressed. *Contra negantem principia non est disputandum.*

But I proceed to relate the observations.

On the 25th of July 1753, being informed, that a calf, come to full maturity was just then brought forth dead in this town (Brigg, in Lincolnshire), which had been alive, and appeared strong a very short time before its birth. I begged it of the owner, such instances being rare. The skin being of value, for it was an extraordinary large calf, it was sent to my house stay'd. I first examined the thorax, which was my chief motive for begging it. All I shall mention now of what I observed there, is the experiment of the lungs of a new-born animal their sinking in water. After cutting out the lungs and heart, I clipped off a piece of the former with sharp scissars, about an ounce weight, or more, and threw it into a basin full of water. It quickly sunk to the bottom, and settled there. Immediately after, I blew into the remaining part of the lungs, through the trachea; and though I could by that means distend them but very little, because the air flowed out readily through the cut bronchia, and therefore acted but faintly

faintly on the other parts; yet a piece about the same bigness with the first, clipped off in the same manner, and thrown into the same basin, constantly kept at the top. This may seem foreign to my present purpose; but I thought proper briefly to mention it here, not only upon the account of the importance of the experiment, but likewise to shew, that I was not misinformed in the account of the calf's being brought forth dead, and that it had not even respired; much less taken any nourishment after exclusion, to influence the appearances, which we are now going to describe.

Having done with the thorax, I opened the abdomen, in order to take a slight view of it before the lumbering carcase should be removed. I observed the thick intestines, especially the rectum, extremely distended with an incredible quantity of meconium; which for several inches above the anus was formed into distinct scybala or balls. It came into my head to make an incision in the rectum, where it was very turgid, about two inches from the anus, and to let out about twenty-five or thirty of these scybala; which I laid upon clean paper to dry, that I might examine them at my leisure. About three or four days after, when they were dry and brittle, of the colour and consistence of aloes; I was surprised to find, upon examination, every ball stuck full of tough, thick, white hairs, some of which were an inch long, or more. There seemed to be some scores in each, though they, being shrunk with drying, did scarce exceed the bulk of an ordinary pea. This unexpected appearance set me a considering, whence these hairs had come; how got they there? And I

could think on no other tolerable solution of the difficulty than to conclude, that they belonged originally to the calf's skin; and, being loosened by maceration in the liquor amnii, were propelled into the stomach and intestines; till they were at length entangled in the meconium. I was confirmed in the belief of this by being informed, upon inquiry, that the calf's skin was white; a circumstance unknown to me before, it having been sent flay'd, as hath been already said.

From this persuasion it was natural to infer, that if hairs loosened from the skin of the fœtus, and floating in the liquor amnii, can find a way into the intestines, and get entangled in the meconium, it is impossible but the liquor amnii must enter and pass through the whole alimentary passage along with them; as a fluid may certainly penetrate where hairs cannot: but no good reason can be assigned, or even conceived, why hairs should be admitted where the fluid is excluded.

The only reasonable scruple, that remained to be got over, was, that this being but a single instance, a general conclusion was not to be too hastily drawn from it; that it was possible there might be some morbid concretions in the meconium of this particular calf, resembling hairs, which concretions in a common and natural way might be wanting; or some preternatural communication between the primæ viæ in this subject, and the liquor amnii, not to be found in the generality of other fœtus's.

To obviate all this, and farther to establish and illustrate the truth of my conclusion, as instances like that, which we have now related, cannot be commanded,

manded, I laid myself out to procure the first dung of calves after they are brought forth ; which always can be nothing but meconium. This was attended with some difficulty and delay, as cows in this neighbourhood mostly calve in the fields. After some months, by the good offices of a sensible acquaintance in this place, I obtained what I wanted. He was so obliging as to watch his own cow's calving, and before he stirred from her, to catch the calf's first dung, and send it me forthwith. Some weeks after another parcel of the same kind was sent me from another quarter. In both which I found a great number of strong hairs all over ; so as to leave no room for doubting but that this appearance is general in the meconium of calves, in a natural way.

The reader will please to observe, that in neither of these instances I could be deceived, if I had ever so little reason to trust to the judgment and fidelity of those, who supplied me with what I wanted. The colour and consistence of the meconium of a fœtus is so very peculiar, and so widely different from that of fæces formed out of ingested aliments, that none, who have any knowledge in these matters, can mistake the one for the other.

In the mean time I omitted not to open the embryo's of the cow-kind, such as I could procure in the shambles of the market-town I live in, and to examine their meconium. The two most advanced towards maturity, which I met with, had stiff long hairs about the mouth, the eye-brows, the ears, and navel, and a good many on the end of the tail ; but none on their skins. In neither of these, any more than in the younger embryo's, which I examined,

was there so much as a single hair to be found in the meconium; for this plain reason, if I judge right, to wit, because they had not got hairs upon their bodies of long enough continuance to become loose, and float in the liquor amnii.

But as opportunities of coming at fœtus's of this species, especially such as are remarkably nearer to maturity than those two I have just now mentioned, are rare with us, I tried to supply that defect by opening those of other animals. Accordingly I procured six puppies, of the butcher-dog-kind, brought forth at the full time, at one litter. Having taken out the whole meconium of every one of them, after the strictest search, I could find no hairs in any part of it. I had likewise an opportunity of opening a colt, that died either in the birth, at the full time, or immediately after, before its meconium was discharged; which I found in great quantities in its rectum and colon. But neither here could I spy a single hair, though I examined whole pounds of it, and that portion thereof most carefully, which was lodged in the rectum, near the anus.

These observations may seem at first view to clash with and contradict those I have related: But, upon closer consideration, they will be found in reality to confirm them, for this reason, to wit, that puppies and colts, when brought forth, have no loose hairs on their bodies; but calves have in great numbers. In the puppies and colt, which I examined, the hairs were so firmly rooted on their skins, that I could scarce pull any off with my thumb and fingers; whereas in a ripe calf, new brought forth, many are found quite loosened at their roots, and only adher-
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ing to their skin by the moisture on it. Therefore in the latter species, hairs from the surface may be, and actually are, incorporated with the liquor amnii, and along with it enter the mouth and alimentary canal, which cannot be the case in the former.

From these facts it is easy to draw such inferences, as are sufficient to decide the controversy, which we are treating of; and prove incontestably, that the liquor amnii is in a constant natural way received into the mouth, stomach, and intestines, and therefore must contribute to the nutrition of the fœtus.

For if we attentively consider, that in the meconium of an human fœtus no hairs are found, there being no loose hairs on its skin when it is born; that in the meconium of unripe calves there are none, before they are of so long continuance on their bodies as to be loosened, and float in the liquor amnii; whereas in that of the ripe calves they are found in great abundance, of the same colour with those of the skin, as appears by the example we had occasion to examine most particularly; and of such a length, stiffness, and thickness, and so constantly to be met with, as cannot with any shew of reason be imputed either to morbid concretion, or preternatural frame: If, I say, all these facts are attentively considered, and duly weighed, the conclusion I have drawn will, I hope, appear to follow from them so naturally, that no candid and intelligent inquirer will call its truth in question.

P O S T S C R I P T.

After I had gone thus far upon my own bottom, and while I was looking about me, in order to pick
up

up more proofs, that might, as it were *ex superabundanti*, confirm and illustrate the doctrine, which I was endeavouring to establish, before I should venture to publish it, I casually found what, as I here solemnly declare, I knew nothing of before, to wit, that I was not the first, who had observed hairs in the meconium of ripe calves, two authors having been before-hand with me, to wit, Aldes and Swammerdam; the former in his *Epistola contra Harveium*, published in the first volume of the *Bibliotheca Anatomica* of Mangetus and Le Clerc: the latter in the *Biblia Naturæ*. Aldes, a feigned name under which Slade, an Amsterdam physician, conceals himself, barely mentions the observation, without drawing any inference from it, either on the one side or the other. Swammerdam draws the same conclusion with me; but, I think, he goes farther than the observation can well justify, maintaining, that it evidently follows from thence, that a calf licks his own skin *in utero*, thereby loosening the hairs, and swallowing them down along with the liquor amnii, its nourishment. See the whole passage, *Bibl. Natur.* pag. 319.

However that be, it is enough for my purpose to have two such authors concurring with me, as to the reality of the fundamental fact, and quite saves me the trouble of making any farther trials to confirm it.

It is no wonder, that these passages escaped me, who have spent almost all my time in practice, and practical studies, and who never was possessed of either of these books; but, after I found them quoted, was obliged to trouble two learned friends, at a di-

stance, upon this occasion, for my own satisfaction, and that very lately. The one was so obliging as to send me the *Bibliotheca Anatomica*; the other to write out at large the passage from the *Biblia Naturæ*. I say, it is no wonder the passages escaped my notice, since they seem to have been overlooked by not only all the particular writers on the fœtus, since Aldes and Swammerdam, but likewise, for aught I know, by all the lecturers on physiology in Europe, preceding August 1753; at which time I began to communicate by letters, what I then took to be a discovery, to several correspondents versed in these matters, both at home and abroad, as I can vouch by their answers.

So that I hope the public will do me the justice to believe, that though I was not the first in point of time, that made the fundamental observation, yet I was obliged to no author for it; and that I have drawn an important truth relating to the animal oeconomy out of obscurity, and established it by supplemental observations, as well as by reasoning.

Before I conclude, let me acquaint the reader, that an ingenious student of anatomy and surgery *, to whom I had communicated my first observations, and recommended to him to examine the liquor contained in the stomachs of ripe new brought forth calves, if he had a proper opportunity, acquainted me last harvest, that he had not long before actually found hairs in abundance in the liquor contained in

* Mr. John Clark, now surgeon to the Lock-Hospital, near Hyde-Park Corner.

one of the stomachs of a calf, brought forth dead at the full time, of the same colour with those on its skin. As this observation renders the proof, however clear and unexceptionable it was before, fuller, I was unwilling to omit it here.

Brigg, July 12,
1755.

Malcolm Flemyng.

XLIII. *An Account of the Success of Agaric in Amputations, &c. in a Letter from Mr. William Thornhill, late Surgeon to the Infirmary at Bristol, to Robert Dingley, Esq; F. R. S.*

Novem. 2, 1755.

Read Nov. 13, 1755. **I** Have made use of the agaric in four amputations: the first was on the arm, between the elbow and wrist; the second and third were below the knee; and the fourth on the crural artery. In every case it answered beyond my expectation; the patients had little or no fever, and no spasms at all, which are frequently the consequences of the ligature, and sometimes fatal. I am surprised, that any objections are made to the use of it. I fear the true reason of its not being used in general practice proceeds from a narrow selfish way of thinking; but it is my opinion it will make its way, to the honour of its first author. I am well satisfied, it is the best medicine in all the *materia medica* yet discovered for that purpose.

I have